

**Can Constructed Tidal Wetlands Contribute  
Phytoplankton to Delta Channels? A Case  
Study of Chlorophyll-a flux from the Decker  
Island Restoration Project**

**#0036**

# Technical Panel Review

**Proposal Name:** Can Constructed Tidal Wetlands Contribute Phytoplankton to Delta Channels? A Case Study of Chlorophyll—a flux from the Decker Island Restoration Project

**Applicant Organization:** United States Geological Survey

**Principal Lead Investigator(s):**

Brown, Larry  
Shellenbarger, Gregory

**Amount Requested:** \$232,007

**TSP Panel Summary of Findings:**

This project could add incremental knowledge to a priority area for CALFED - namely, the ability to identify chlorophyll a flux between a restored Delta tidal marsh and its adjacent delta channel. It addresses a key question about phytoplankton production in restored tidal marshes: how much do these systems contribute to the phytoplankton-based aquatic food web? However, its approach has several shortcomings identified by the external technical reviewers relative to the biological processes, all of which could be overcome with study design modification that would entail a more complex and costly project. One particular shortcoming relative to the stated hypothesis is that the study does not account for any other factors that may influence the phytoplankton exchange between the restored marsh and its external Delta channel and thus its power to explain its results is very low. What if the site yields minimal output? Does it mean the site is not producing phytoplankton or does it mean that internal grazing consumes all that is produced? With this low explanatory power, the project becomes a protocol development effort for measuring tidal channel discharge, which does not necessarily rise to the level that CALFED is looking for. The other aspect of the approach, discharge measurements, are confusing. The equipment they propose to use appears to be described incorrectly relative to the manufacturer's web site product information (or it is an old model) and the description of its application is incomplete, making it difficult to interpret how they plan

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to measure discharge and thus whether the approach is feasible or not. The Decker Island site is a good place to carry out this work, because all discharge goes through a single channel entrance without confounding variables of direct marsh overbank flow.

What is the applicablility of this project to the rest of the Bay-Delta? This research focuses on Decker Island, which is not a natural tidal wetland in the Delta. Thus, information provided will have limited applicability to other systems, but still may prove useful.

**Recommendations:** It would be ideal for the researchers to examine a couple of different sites and use additional sampling methods.

### *Relevance to PSP Topic Areas:*

Moderate

### *TSP Technical Rating:*

Sufficient

### *TSP Funding Recommendation:*

Do Not Fund

*TSP Amount Recommended:* \$0

### *Conditions:*

# External Technical Review #1

**Proposal Title:** Can Constructed Tidal Wetlands Contribute Phytoplankton to Delta Channels? A Case Study of Chlorophyll--a flux from the Decker Island Restoration Project

**Proposal Number:** 0036

**Proposal Applicant:** United States Geological Survey

## Purpose

Comments	The question that is addressed by this proposal--whether tidal wetland restoration projects influence phytoplankton production in the Delta ecosystem--is of only limited interest. Conceptually it seems uninteresting and might only be of interest at an extremely local level, but even there it is not sufficient to lump all phytoplankton together as if they're a single organism, or even type of organism. Since many grazers are specific as to what they can ingest and/or digest, lumping all phytoplankton from an ecosystem perspective is simplistic and not likely to lead to a meaningful set of conclusions.
Rating	Sufficient

## Background

Comments	The background provided in this proposal strikes me as rather thin, and focuses primarily on how tidal mixing and other physical forcing functions may influence phytoplankton biomass and movement. There is little in the way of identifying those chemical and physical factors that directly affect phytoplankton growth--light, salinity variations, concentrations of specific nutrients that may limit primary production, etc.
Rating	Sufficient

## Approach

Comments	While the PIs are competent and focused, this proposal does not strike me as being particularly well thought-out. The approach, as I understand it, will involve measuring chlorophyll a levels. But since that pigment is common to all phytoplankton types, it is only useful as a proxy for total phytoplankton biomass, rather than composition, which would surely affect ecosystem processes more. Thus, we will have no clear understanding of how restoration efforts will influence the composition of the phytoplankton communities present. This can be roughly determined through analysis of auxiliary photosynthetic pigments, but I did not see any reference to proposed work of this kind.
Rating	Inadequate

## Feasibility

Comments	No, for reasons noted elsewhere in this review.
Rating	Inadequate

## Budget

Comments	Seems ok.
Rating	Sufficient

## Relevance To CALFED

Comments	The PIs have attempted to put together a proposal that is seen to be relevant to CALFED's needs. With some appropriate modifications I suppose the proposed work could help CALFED in understanding the ecosystem implications of their restoration activities, but this strikes me as pretty coarse and not
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### External Technical Review #1

	sufficiently detailed to provide real meaningful data that would help CALFED in the long run.
Rating	Sufficient

### Qualifications

Comments	No problem there.
Rating	Sufficient

### Overall Evaluation Summary Rating

Comments	See above.
Rating	Inadequate

# External Technical Review #2

**Proposal Title:** Can Constructed Tidal Wetlands Contribute Phytoplankton to Delta Channels? A Case Study of Chlorophyll—a flux from the Decker Island Restoration Project

**Proposal Number:** 0036

**Proposal Applicant:** United States Geological Survey

## Purpose

Comments	<p>Are the goals, objectives and hypotheses clearly stated and internally consistent? • Goal and objectives are relatively clear as stated. • Null hypothesis seems a bit 'token', if my interpretation is correct that the only way that their hypothesis can be accepted is that there is absolutely no effect (either sink or source) of phytoplankton production from Decker Island to the overall Delta.</p> <p>Is the idea timely and important? • Given the concerns revolving around the POD, this would appear to be a timely project.</p> <p>Is the study justified relative to existing knowledge? • To a large degree, the existing knowledge that is available is effectively exploited to establish the logic behind the hypotheses. There is more recent work on the sources of organic matter driving Bay-Delta food webs that suggest it isn't all phytoplankton, but this is very recent results from the CALFED IRWM program that has not been published yet. Irrespective, the hypothesis still holds and may be generally valid for the Delta (as compared to the Bay). • However, the proposal is poorly substantiated by the state of the scientific literature, particularly relative to the physics and analytical approaches involved in measuring particle fluxes in tidal channels, e.g., Lu and Lueck 1999a (J. Atmos. Oceanic Technol, 16 : 1556-1567 &amp;1568-1579); Lu et al. 2000 (J. Phys.</p>
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## External Technical Review #2

	<p>Oceanogr. 30: 855-867); Lanzoni et al. 2002 (J. Geophys. Res., 107 (C1), 10.1029/2000JC000468).</p> <p>Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified? • The project is appropriately scaled around the peak 4-month phytoplankton production period, with the potential to scale that up over a longer time period and multiple years (under new grant) if the results indicate a clear trend that justifies a broader understanding of seasonality and variability.</p> <p>Are results likely to add to the base of knowledge? • Yes, flux rates are notoriously difficult to estimate and if carefully implemented this project could considerably advance our state of understanding.</p> <p>Is the project likely to generate novel information, methodology, or approaches? • The project is not particularly novel, although it takes good advantage of state of the science technology.</p>
<p><b>Rating</b></p>	<p>Above Average</p>

## Background

<p><b>Comments</b></p>	<p>Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? • To a certain degree. It does lack the usual description of the physical and biological processes that could be operating to affect the fate of phytoplankton and suspended sediment within the site, between major tidal and freshwater flooding cycles. For instance, there is no indication of settling and deposition to the benthos, other than an arrow going to benthic feeders. • Mechanisms and regulating factors are poorly discussed. For instance, there is not indication that the density of <i>E. densa</i> (and thus the modulation of currents, resuspension of diatoms, etc.) is taken into account. This would potentially limit the exportability of the results and</p>
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## External Technical Review #2

	<p>approach (Secondary Objective #1). And, it is unclear whether phytoplankton biomass is, in fact, the critical metric rather than productivity/cell loss (including grazing) inside the flooded island. If these mechanisms and processes were spelled out in the conceptual model, it may have been more evident that primary production measurements inside and outside of the project might increase the information value of this research considerably.</p> <p>Is all other information needed to understand the basis for the proposed work included and well documented? • Although the proportional representation of indigenous vs. non-indigenous fishes is relevant to the logic behind formulation of the hypothesis, it is not really material to the goals and objectives and a bit distracting from the point of the proposal. (But, it is interesting that the data presented in Table 1 seems to suggest that, even though non-indigenous species swamp both the fish density and taxa richness metrics [Figs. 2 &amp; 3], the indigenous fishes tended to occur in higher abundances inside the project compared to outside at the reference site? Does this make for better habitat or a nuisance?)</p>
<b>Rating</b>	Above Average

### Approach

<b>Comments</b>	<p>Is the approach well designed and appropriate for meeting the objectives of the project? • My impression as a non-expert (which, hopefully, other reviewers will compensate!) is that the approach and methodology descriptions do not provide enough information to ensure that the flux estimates will be accurate. My impression is that acquiring accurate flux estimates from tidal channels is not a simple task. If the tidal currents are at all asymmetrical (as would be expected; Ganju et al. 2005), there is any density stratification, and the vertical and</p>
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## External Technical Review #2

cross sectional variation in velocities and vertical mixing are not uniform, it is difficult to understand how a vertically averaged measurement of flux will be accurate. In addition, the use of the upward facing ADVm would not capture much of the near-bed shear and most turbulent portion of the velocity field, particle flocculation, settling and resuspension, etc. The proposal does not provide any estimates of the potential magnitude of this bias and error, so it is difficult for the non-expert to understand whether it represents a significant factor of uncertainty in estimating net flux. For example, given that Ganju et al. 2005 were unable to close the suspended sediment budget for the net flux at nearby Browns Island due to a combination of relatively uncontrollable groundwater seepage, overland flow, and flow through minor channels, how do these authors expect to make differentiate a potentially more subtle difference with comparable or, seemingly, less rigorous measurements? Thus, although the approach may not be flawed, I am not sure that the authors provide enough information to ensure that it isn't?

Is it clear who will be performing management tasks and administration of the project and are resources set aside to do so? Are products of value likely from the project? • Yes, responsibilities are relatively well defined.

Is there a plan for widespread and effective dissemination of information gained from the project? • A list of committed and potential reports is provided, but submission of a manuscript to a peer-reviewed scientific journal is contingent.

## External Technical Review #2

	Are contributions to larger data management systems relevant and considered? • No, data management does not appear to be materially considered.
<b>Rating</b>	Inadequate

### Feasibility

<b>Comments</b>	<p>Is the approach fully documented and technically feasible? • Although it appears to be technically feasible, given the experience in using these technologies, some additional aspects of the methodology would have made it a bit more convincing (e.g., amount of cross section and vertical water column covered by the sensors (see above).</p> <p>What is the likelihood of success? • The measurements are likely to be successful. Whether the flux rates will be accurate is perhaps another question.</p> <p>Is the scale of the project consistent with the objectives and within the grasp of authors? • Yes, the project appears to be scaled according to both objectives and the investigators capabilities.</p>
<b>Rating</b>	Above Average

### Budget

<b>Comments</b>	<p>Budget Is it clear how much each aspect of the proposed work will cost including each task, salaries, equipment, etc.? • Yes, the tasks are well defined and described.</p> <p>Is the budget reasonable and adequate for the work proposed? • The overall budget is reasonable, albeit with a very hefty overhead.</p>
<b>Rating</b>	

## External Technical Review #2

	Sufficient
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### Relevance To CALFED

<b>Comments</b>	<p>How well does the proposal address the priorities stated in the PSP? • Only partially, especially from the standpoint of exploiting existing information.</p> <p>Does the proposal clearly and directly address one or more of the topics in the Priority Research Topic List? • Not exceedingly well; many of the rationale are tangential.</p> <p>Does the proposal address other priorities stated in the PSP such as integration, syntheses, use of existing information, multiple disciplines or modeling? • Poorly, if at all.</p> <p>Will the information ultimately be useful to CALFED resource managers and policy makers? • As a pilot, exploratory study perhaps. It will unlikely be conclusive.</p>
<b>Rating</b>	Above Average

### Qualifications

<b>Comments</b>	<p>What is the track record of authors in terms of past performance? • The performance of the authors has been reasonable.</p> <p>Is the project team qualified to efficiently and effectively implement the proposed project? • Technically, the team should be able to achieve the objectives of the field experiments. However, their analytical and interpretive experience in flux measurements seems a bit weak, especially in the case of the lead P-I. It seems very odd that Dr. Shoellhamer, the lead USGS expert in the field, would not even be an advising P-I on this project?</p>
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## External Technical Review #2

	Do they have available the infrastructure and other aspects of support necessary to accomplish the project? • USGS and other partners should be able to provide all the necessary support to accomplish the project.
<b>Rating</b>	Sufficient

## Overall Evaluation Summary Rating

<b>Comments</b>	Sufficient to below average: Although this proposal is designed to address an interesting, and relatively important question about the potential role of restoring wetlands to the Bay-Delta (although it might be argued that it would be an 'ephemeral conclusion' if the site evolved into a vegetated tidal wetland in any reasonable timeframe?), the approach and methodology seems to be insufficient to meet the fundamental objective (reliable, accurate flux rate estimate sufficient to assess 'sink' or 'source') unless the signal to noise ration is phenomenally high.
<b>Rating</b>	Sufficient

# External Technical Review #3

**Proposal Title:** Can Constructed Tidal Wetlands Contribute Phytoplankton to Delta Channels? A Case Study of Chlorophyll—a flux from the Decker Island Restoration Project

**Proposal Number:** 0036

**Proposal Applicant:** United States Geological Survey

## Purpose

Comments	The goals, objectives and hypotheses are logically consistent. Of course, the research matter is timely and important but I think the scale of the study is somewhat limited. The ultimate goal of this study is determine if tidal wetlands are a sink or source of phytoplankton production is quite interesting, however, by only selecting one site, I wonder if the hypothesis is too site specific? This study would be much more interesting and relevant if researchers selected at least 2 sites of differing wetland configurations to determine if the sink or source phenomena exists on a greater spatial scale. Work by Lucas et al. in the estuary suggests that submerged wetlands of differing configurations (i.e., deep or shallow) and hydrology function quite differently with respects to phytoplankton production and export. By only looking at chlorophyll a influxes and hydrology, the researchers are also ignoring a time-scale grazing effect by primary and secondary consumers, which, can also vary in composition and abundance in any given month or year.
Rating	Above Average

## Background

Comments	The conceptual model makes sense and has clear POD connections. Determining if wetlands act as sources or sinks for phytoplankton and/or sediment does help answer the question about both the connection between
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### External Technical Review #3

	food production and pelagic fish abundance in the delta. However, I think by only measuring chlorophyll a and sediment influxes, the research team is missing some critical links that need to complete their argument about whether source-sink dynamics matter (see below).
Rating	Above Average

### Approach

Comments	<p>The two biggest weaknesses I see in the proposal are the following:</p> <p>1. Pelagic consumer measurements of abundances. 2. Macrophyte (i.e., <i>Egeria densa</i>) measure of abundance.</p> <p>1. There is the possibility that Decker Island is a source of production for pelagic consumers, but if pelagic consumers uptake the majority of the phytoplankton biomass within the site and subsequently move out the wetland than the wetland can be considered a source but this would not be reflected by measurements of chlorophyll alone. I believe some measurement of zooplankton (at the very least) needs to be made to estimate biomass-grazing terms in the export model. I think it would be fairly easy to add a pelagic consumer element to this study (i.e., mid-channel net sampling).</p> <p>2. Suspended sediment inputs and exports will probably vary with SAV abundance. Measurements of SAV abundance (i.e., estimates of biomass) should be made to determine its effect on suspended sediment loads.</p> <p>I have one minor comment about the SCUFA's ability to distinguish between different phytoplankton. It would be nice to know which phytoplankton are dominating the chlorophyll a signal, in other words, is the good diatoms or something else less palatable, such as microcystis. I don't think it would be hard for the research team to verify at least a few samples.</p>
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### External Technical Review #3

<b>Rating</b>	Sufficient
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### Feasibility

<b>Comments</b>	The approach is feasible for the objectives listed in proposals but overall the scope is limited and I think the research team will provide just a small piece of the puzzle regarding the greater source-sink question in the Delta.
<b>Rating</b>	Sufficient

### Budget

<b>Comments</b>	The budget appears to be reasonable and adequate. I would like to see a few added elements and a larger budget
<b>Rating</b>	Above Average

### Relevance To CALFED

<b>Comments</b>	The proposal definitely addresses a priority stated in the PSP. Once again, the study is missing a few key elements to be really useful to managers and policy makers. I would like to see if the team can add these elements.
<b>Rating</b>	Sufficient

### Qualifications

<b>Comments</b>	Excellent.
<b>Rating</b>	Superior

### Overall Evaluation Summary Rating

<b>Comments</b>	This proposal has merit but just falls short of answering some key source-sink dynamic questions. Namely, I would like to know:
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External Technical Review #3

	<p>1. If wetland configuration (depth, SAV colonization, hydrodynamics) contributes to source-sink variability. I would like to see the research team add another site.</p> <p>2. If consumers are using phytoplankton biomass in the wetland and than moving it out to upper trophic levels. Measuring pelagic consumer abundance would be good element to add to this proposal.</p> <p>3. How does sediment influx vary with direct measurements of SAV abundance? Measurements of SAV biomass would suffice here.</p>
<b>Rating</b>	Sufficient